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DISCUSSION OF THE SPECIES QUESTION.

AT THE close of Mr. Cowles's paper the chairman invited those present to discuss the question. The following remarks were made:

MR. J. M. COULTER: I find myself in some particulars agreeing with every speaker, and it seems to me that there are enough elements in common in all that has been said to enable us to reach some sort of working basis. One thing agreed upon in all that has been said is that nature makes individuals and men make species. This means that in a certain large sense the individual is the unit to be recognized. At the same time it is evident that the attempt to record such units on the basis of any present scheme is entirely out of the question. While it is clear that we are approaching greater uniformity in our conception of species, we shall not reach agreement until we know more definitely the influence of ecological and physiological factors, if we may separate them for the moment, upon structures. But until this knowledge becomes more exact, how shall records be made? We must have some temporary and effective method. We all appear to recognize the fact that the indefinite multiplication of names of species must be stopped sooner or later, and the sooner the better; for we are getting the record into a condition that makes it unworkable. It seems to be the belief of every speaker that something must be done, and that the conception of a species must be modified eventually. It is quite evident that we are in no position as yet to formulate definitely what we shall agree to call a species, and in the meantime it seems to me the suggestion made by Dr. Britton is a very workable one. It gives to every one the opportunity to distinguish and record forms clear down to individuals if he chooses; and at the same time

it removes the grievous burden of a great multiplication of names. It seems to me that the suggestions of Dr. Bessey and Dr. Britton almost meet at this point. In effect, it means to continue to name easily recognized forms, calling them species if desired; and then by a system of numbers to indicate the more refined distinctions. This avoids the great multiplication of names and secures an exact record. This method has been developed so effectively in the cataloguing of stars and books that it could be adapted for plants without serious trouble. I would ask Dr. Britton how much this scheme would reduce the number of published names.

MR. N. L. BRITTON: I do not know. Those that have been published up to this time it would reduce probably two thirds.

MR. J. M. COULTER: A reduction of the names to one third would be a good start, and I think we had better hold Dr. Britton to his idea. I think we can get together on this suggestion, so that all botanists can recognize the ordinary forms and remember their names, and the taxonomists can record the more refined distinctions. I had not heard of this proposition before, but at present it appears to me to be desirable and workable.

MR. J. B. POLLOCK: I should like to say a word along the line of work upon which Dr. Arthur has presented some thoughts to you, but I think we can go a little further than Dr. Arthur did in his argument in regard to a definite method of getting at species from the physiological basis, and the people who are nearly ready to begin that, I feel sure, are those who have worked with the fungi from the cultural side. In talking with a number of men, who are interested in that side of the work, in the course of these meetings, I have been impressed with a fact the importance of which has been growing upon me in my work alone, that the men who are doing that kind of work are feeling the absolute necessity of doing something definite exactly along the line which has been indicated by almost all the speakers here in a general

way. Now, what I mean is the kind of work which was reported in the botanical session this morning on *Penicillium* and its relation to definite culture media, and I believe in the very near future we shall be describing species, of many of these fungi in a definite relation to culture media. A species will be a group of those organisms which show a certain combination of morphological characters when grown upon a medium of definite and known composition. The type of the species involves a type medium, and the fluctuating variations must be determined by the use of a series of such media. Considerable work in this direction has been done, but the method has not yet been well adapted to taxonomic description. I think we are ready now to do this work from a strictly taxonomic point of view. That will be doing exactly what at least four of the speakers have advocated, but they did not tell us in detail how to do it. These fungi are particularly suited to this kind of work for several reasons. In the first place, they are small in size and we do not need a garden in which to grow them; we can grow them in a test-tube. In the second place, they reproduce exceedingly rapidly and we do not have to wait a year or ten years in order to get them to produce new generations. They may do that in a few hours, or at most in a few days, so that we can produce generation after generation, while the man who is working with flowering plants is waiting for one generation to develop. Hence these plants are favorable material with which to begin the kind of work which seems to be demanded, namely, the union of the morphological and physiological factors in the description and delimitation of species.

MR. T. J. BURRILL: I do not know that I have anything very definite to say, but I am somewhat conscious of a feeling that I am glad that I am not a taxonomist this afternoon. The taxonomist has certainly suffered this half day. It was said here a few moments ago that there had been no progress along the line of the systematist;

that we have developed greatly elsewhere, but in this particular we have remained medieval or worse than that. I think we have abundant evidence right here that there has been progress. This meeting this afternoon would have been no more possible a dozen years ago than it is now to separate out the species of *Cratægus* by somebody who has not studied them during that time. There has been something working. People have been finding that there is something besides names and dates; something has occurred since 1753. It looks to me as though it were hopeful. If there is anarchy, more of that than anything else, it is a pretty good beginning to start something that is not anarchy.

However, I feel sure I shall not give up everything in regard to our common notion of species and species' names. I do hope that I shall know the white oak under its proper name hereafter when I see it, and a few other things like that; and I think it is really important that when we speak about certain things, of certain effects and relations, we do understand what we are talking about and that others may also understand. I think it does make a difference as to whether we continue the use—to take Dr. Arthur's first illustration—the definite use of such terms as *Bacillus anthracis* and *Bacillus typhosus*, and the rest of them. If a writer is going to discuss a subject in which these organisms play a part and one that is vital to our existence and welfare, it is surely desirable that we should have a correct interpretation of what he means when he uses these terms. I believe we are pretty near that meaning in regard to the common organisms in the group of bacteria, so that we know pretty well what is meant when such names as those above are mentioned, though nobody will deny that included among the organisms to which the names apply there are considerable differences. They are different in their sizes, if you come down to microbe measurements; they differ certainly in their growth upon different media, and I do not see why the physiological

effects may not just as well enter into the species idea as the morphological characteristics, if we can get them; but I do not know how we are going to get them for such plants as forest trees. In the case of the white oak, we have got to take the morphological characteristics alone, so far as I see. We can not plant the seeds and witness the trees as they develop in their various stages until they produce seed again. But in case of the lower forms with a generation a day, we can certainly do that, and I think we ought to do it for specific characteristics.

Let me say, what I am most interested in is that we shall know some things yet by some very well known names.

MR. E. G. HILL: As we have had considerable talk from the pulpit, perhaps a little experience from the pew in regard to species might be of use. In 1874, I came to that part of Illinois which is now embraced in the city of Chicago, and the year after I began the study of the flora of the region, the sand region (it was all sand then), particularly this dune region east of the city, and there has not been a year since that I have not been out there more or less. Now, I have not been over every foot of it, but with regard to this plant, the *Polygonum amphibium* that has been cited, I always until about 1890 or 1895, I can not give the exact year, I always saw *Polygonum amphibium*, I never saw the other kind. At that time I noticed this hairy plant, with shorter internodes, growing on dryer places. The exigencies of the expanding city and manufactures outside of it led to the partial drainage or entire drainage of considerable areas of our dune flora. The underground parts of *Polygonum amphibium* are pretty extensive. I never satisfy myself without digging up the entire under part of plants, or digging up the root—I can follow a root ten feet, if necessary. When those wet places were drained, the roots persisted in the sand, and the plants while preserving their full form when in the water, would be hairy, and you can trace them all the way from the water up,

may-be twenty to thirty feet from the edge of the water, making their way even along paths or roads. Now, I want to say, I do not know that they did not exist before that, but I never noticed them until then; and not a mile or two miles from here, if there are vacant lots, you will probably find *Polygonum hartwrightii* in a drained slough, where twenty to twenty-five years ago you would not find anything of that kind, but you would find *Polygonum amphibium*. It seems to me that is the way in which it might have originated here. This experience extends from about 1875, and although I gather from it that it was not here before, I believe that it made its appearance not far from 1890 to 1895.

MR. G. H. SHULL: In one sense the recent discoveries in the realm of variation and heredity appear to lead to a backward step in the concept of a species, since the view of the naturalness of species to which these discoveries lead, resembles the earlier view. Upon a comparison of the basis of this conception, however, in the time of Linnæus, with the present situation, the contrast becomes sufficiently striking.

At that time no cognizance was taken of the importance of variations. When variations were first fully taken into account, about the middle of the last century, they were apparently conceived to have no natural limits, and in consequence, no form-group could have natural limits.

The demonstration that variations have natural limits has shown that the lowest grade of form-group is a *natural* group of individuals differing from each other only by fluctuations. It appears to me that the able discussions to which we have listened might all be reduced to the question of the desirability or feasibility of looking upon these *ultimate, natural form-groups as species*. This question largely rests upon the question of utility, and herein lies our difficulty in reaching a universally satisfactory conception. To the maker and keeper of herbaria and to the field naturalist, utility requires that species shall be separable by characters which may be

recognized by a more or less careful examination of a single individual of unknown ancestry. To the experimentalist, whether he be a physiologist, an experimental morphologist or a pedigree culturist, such a conception of species makes them of no utility, and this is also largely true with students of cytology, anatomy, etc., who study the morphology of structures not externally visible. As a student of variation and heredity from the experimental side, I hold that the *natural* group is the only one that can be of any utility to any one but the herborizer, but I do not insist on the privilege of calling this natural group a species. That matter is entirely immaterial. To avoid confusion, we are now calling these groups elementary forms, but this expression is unnecessarily cumbersome for the constant use the conception must have in the future literature of botany, and I hope that some one will come forward with an acceptable short designation for these, in case the herborizer succeeds in checking the trend of development of the conception of species from an arbitrary to a natural one.

Experimental evidence at the present time shows at least two grades of form-groups. These were recognized by Dr. De Vries under the names, elementary species and varieties. By "varieties" De Vries indicates those forms which differ from their nearest related form in one or more characteristics, which behave according to Mendel's law upon crossing with it. As taxonomists have definitely abandoned the use of the term "variety" to the horticulturist, we need a new word with which to speak of this very definite class of forms. As we have taken liberties with the name of Mendel in the formation not only of the adjective "Mendelian," but also the verb "Mendelize," there seems to be no reason why we might not go farther and call these Mendelian varieties Mendelities. These Mendelities are hangers-on of species, and corresponding Mendelities may belong to many different species, as may be illustrated by the frequency of occurrence of white-flowered or albino forms, which differ

from their corresponding pigmented forms in no other character but that of pigmentation. Whether Mendelities may in any case be the initial modification leading to new elementary species is not known, but I believe that we are not yet warranted in so considering it. But elementary species and Mendelities are certainly natural entities and must be used as the physiological units of form.

MR. J. A. HARRIS: It seems to me that while the systematists are receiving the congratulations of morphologists and ecologists, it will be well for us to remember that he "that is without sin" among us "should cast the first stone." I think in the present state of biology, the morphologist and physiologist and ecologist have some points to learn from the systematist, and that the morphologist, physiologist and ecologist are to some extent responsible for our present inadequate knowledge of species.

The taxonomist has worked out a very careful scheme of recording his observations; he feels himself bound, in preparing a monograph, to cite all descriptions. It is perfectly easy to take a well-prepared monograph and to follow out the earlier literature of any form. If, on the other hand, one turns to morphological and physiological writing, he will find it is a difficult task to locate the work which others have done on the same species. It seems to me that if the taxonomist is expected to make use of experimental and ecological data in his limitation of species, he should be given the data, by those who are doing such work, in a form in which he can use them. Take, for instance, our ecological papers. An ecologist works on a certain region and refers to a number of species definitely, and to a larger number of species indefinitely, grouping them as grasses, sedges, asters, etc. How is the taxonomist to utilize such data. The work of the ecologist should be specific and comparative. He should be explicit in his statements concerning the form and life conditions of a species in his own region, and he should refer to the literature in such a detailed fashion

as will enable the systematist to determine the characteristics and the environment of the same species in another region. I think the morphologist, too, must adopt the practise which the systematist has found indispensable. He should cite in a clear but condensed form all the pertinent morphological literature for each of the species which he treats. The recognition of this obligation would, I believe, do much to raise the standard of our published work, and it would place at the disposal of the systematist a mass of data which he ought to use to great advantage.

MR. A. E. HITCHCOCK: I wish to call the attention of the ecologists to one very important method by which they may greatly aid the taxonomists. The former are doing much careful work in studying the ecological relationships of plants, but in order that this work may be checked up and be available to taxonomists it is necessary that specimens of the species studied should be prepared and deposited in a public herbarium. Unless such a permanent record can be made the statements concerning habitat, variation and other ecological data are of little value to the systematist, because they can not be verified. Such verification is especially necessary because the ecologist is not likely to be sufficiently familiar with all groups of plants to pass authoritatively upon their botanical names. Even when the plants are submitted to taxonomists for identification it may happen that errors occur. Data based upon specimens available in public herbaria would aid ecologists themselves to coordinate their work. Taxonomists will be glad to avail themselves of all serious work done by ecologists when the data can be definitely connected with preserved specimens.

One other point. I wish to emphasize the necessity of field work in determining the limits of species. If possible, taxonomists who are monographing groups of plants should study these plants in the field. The botanist can determine usually to his own satisfaction the limits of variation of a single species in a given

locality. The herbarium then becomes a record of his field observations. Much good work can be done in the herbarium, but when coordinated by abundant observations upon the plants, as they occur in nature, the conclusions are much more likely to be correct.

MR. G. H. SHULL: I would like to say one word in reply to Mr. Hitchcock's remarks regarding what we know about variations when we seen them in the field. It is my experience in the cultivation of things brought in from the field during the last four years, that we know nothing about the significance of variations in the field as to their bearing upon the question of the species, if by species we mean a continuously variable group, because in some instances we find that very distinct variations as seen in the field are immediately lost when the individuals possessing them are grown under uniform conditions. We find, on the other hand, that equally distinct, or even less distinct variations—as determined entirely by an estimate of the difference in form—are retained with absolute permanence so far as one can determine by three or four years of culture under like conditions. Now, how we can tell without an experimental basis for the estimation of the meaning of variation in any particular species, what is the significance of those variations as we see them in the field, I am at a loss to understand.